schools has been already noticed in NATURE (vol. xxxi. p. 19), and the paper before us, by the Principal of the Royal Agricultural College, Cirencester, is another product of the Conferences. The author looks on agriculture broadly, as extending, like the theme of the poet of the "Georgics,"—

> " . . . super arvorum cultu per corumque
> Et super arboribus,"

and in a well-reasoned and well-written paper pleads for the teaching of the natural sciences, their facts, laws, methods, and applications to agriculture, to those who have the direction of agriculture in this country, or who seek fortunes in the soils of new countries. Cowley, two centuries ago, asked, "Who is there among our gentry that does not entertain a dancing-master for his children as soon as they can walk? But did ever any father provide a tutor for his son to instruct him betimes in the nature and improvements of that land which he intended to leave him?" Though this reproach is not deserved so much now as when it was written, it is still not wholly unmerited, and will so remain until those who have the possession and management of landed property shall receive some special training such as that sketched out by Mr. McClellan. This training, if fairly common, would do far more to mitigate agricultural depression than any amount of piecemeal legislation. The paper is a useful addendum to Mr. Jenkins's recent report on agricultural education, and it may be commended to the attention of landowners and others connected with agriculture.

The Text of Euclid's Geometry. Book I., uniformly and systematically arranged. With a discussion of Euclid's application of logical principles, copious notes, exercises, and a figure-book. By J. Dallin Paul, R.N. (Cambridge: Deighton, Bell, and Co., 1884.)

THIS is a "prodigious" work of 182 pages demy 8vo, printed on excellent paper, with clearly-drawn figures, devoted to the "painful" elucidation of all the difficulties to be found in the first book of Euclid's Geometry, with such other matter as hath been adumbrated in the above-cited title-page. The road may be an easy one to walk in, all stones of offence being carefully put on one side or so rearranged that the wayfarer may not stumble as he saunters along it, but it certainly is a long road. The tendency of modern agitation a few years ago was to condense our text-books with a view to get up geometry in the minimum of time, but experience has taught us that in the majority of cases junior boys are very tender-footed, and cannot be driven along the geometrical path, and so there has been a reversion to the "grand old" book with many an aid to lure the young into paths not naturally attractive to them. We do not find fault with these attempts—we have recently noticed in these columns two admirable editions of the "Elements,"—but Mr. Paul has taken, we think, an extreme course: at some perhaps not distant date, if this sort of editing is catching, we shall have a similarly got-up work devoted to Euclid's treatment of isosceles triangles with a preliminary chapter on an axiom.

Our author has had so much to do with Euclid that his views of life have possibly got to be Euclid-tinted, and he sees nothing but Euclid! It would be no wonder, for his own words are, in deprecation of the presumption of adding another edition to the many that have gone before, "having been teaching Euclid almost daily for the last twenty years to pupils who, before coming under his tuition, had learnt something of geometry from the different text-books in use during that time, he ventures to think that this experience has made manifest to him the principal advantages and disadvantages of these numerous works, and thereby enabled him to present the propositions in the form most likely to be of educational value to those who are beginning either to learn or to teach the subject." We have allowed the author to put so much in evidence that the majority of our readers may gather that this is not "just the book they wanted" for themselves, and yet may see the scope of Mr. Paul's labours.

We cannot commend the author's action in placing the notes on the propositions in the early part of the book; experience has shown him that when placed in their usual position at the end they are passed by, but their actual position here offends *our* eye, and will not, we fancy, secure the writer's object. We regret that the writer has spent so much time and thought to so little purpose, as we believe, for we cannot imagine who will be the public that will purchase his book, its size and price are a bar to its introduction into school use. We close with remarking that there is a good deal that may be of use to (say) a pupil-teacher, or to one who is not strong in geometry and yet has to teach young pupils; but much, if not all, of this, can be got in handier text-books. A good feature is the placing at the end the particular enunciations of the propositions with the diagrams placed in positions very different from those which they had in the text: this would enable a pupil to test his acquaintance with the subject. R. T.

Das kleine botanische Practicum für Anfänger. Von Dr. Eduard Strasburger. Mit 114 Holzschnitten. (Jena, 1884.)

A BOOK by Prof. Strasburger, entitled "Das botanische Practicum," has recently been reviewed in NATURE, and recognised as a most valuable addition to botanical literature. The same author has now produced a condensed edition of the same book under the heading given above. The more important of the facts distributed through the 600 pages of the first and larger edition are here collected into the smaller space of 250 pages, an arrangement which is obviously better suited to beginners. It was specially remarked in the review of the larger edition that the efficient study of the various types named would occupy the average student a longer time than the author of the book appeared to think. This smaller edition will obviate the difficulty by supplying the elementary student with a shorter course of study, while the larger book will no doubt be found more useful as a book of reference for more advanced students, or as providing a curriculum for those who will make botany their profession. The merits of good type and excellent illustrations are to be found in this smaller book in as high a degree as in the earlier and larger edition. F. O. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Dr. Koch and the Comma-Bacterium

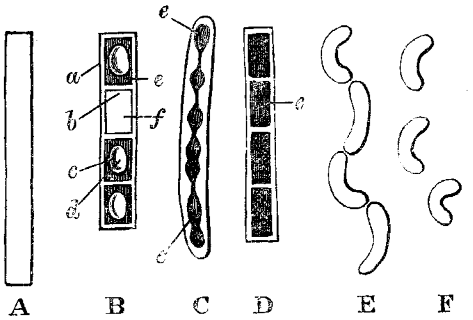
THE article published in NATURE of December 4, setting forth Dr. Koch's well-known theories with regard to the connection of a comma-shaped micro-organism with cholera, serves very efficiently as the text for one who desires to point out the deficiencies in Dr. Koch's observations and reasonings on this subject. The article is the most favourable statement which can be made on the side of those who accept Dr. Koch's conclusions, and is to a certain extent not quite fair to his opponents, since his original statements are not clearly separated from the subsequent statements which he has made in reply to criticisms.

In opposing Dr. Koch's conclusions, it is desirable at the very first to state clearly that those who accept them appear to labour under two important misconceptions, the first being that Dr. Koch is, and has been for a long time, acquainted with *every* form (and the complete history of every form) of Schizomycetes or Bacteria existing, whether in the healthy body or in disease, or in non-parasitic conditions; the second being that no one, with the exception of Dr. Koch and one or two of his pupils, has any real first-hand knowledge of Bacteria which is of any moment. It is hardly necessary to insist in the pages of a scientific journal upon the fact that these really are misconceptions. Our knowledge of the Bacteria is in its infancy—and Dr. Koch's knowledge of them is no more than that which an industrious worker may be expected to have gained in the course of very special observations in regard to a limited class of these organisms (the pathogenic class) extending over a few years. On the other hand, the study of Bacteria has been prosecuted from three separate points of view during the past fifteen years by a number of observers, who may be grouped according to their point of view as the botanists, the chemists, and the pathologists. It is undeniably the fact that neither the chemists nor the pathologists have given much heed to the work of the botanists, and that the results attained by the three groups of workers have not been brought into harmony. To the medical world the special investigations of the pathologists alone are familiar, and undue weight has been given on the one hand to generalisations which ignore the more widely-based conclusions of the botanists, and on the other hand to the introduction into the pathological arena of methods of study which are not new or original, but have been borrowed from the botanists, whose opinions are nevertheless ignored or dismissed with little consideration. As examples of these tendencies I may quote the reiterated assertion by Dr. Koch, and the pathological school, of the conclusion (upon which they base many very momentous arguments) that the forms and the activities of Bacteria are absolutely fixed and limited—that micrococci only produce micrococci, bacilli only bacilli, spirilla only spirilla, and that none of these forms vary from generation to generation, or can be produced from another of these forms, and that a micro-organism producing a particular disease or a particular ferment cannot in the course of generations lose the property of producing that disease or that ferment, and *vice versâ* that one not having such properties cannot, in the course of human experience, acquire them. This axiom of the pathologists as to fixity of form and property, is entirely opposed to the conclusions of the botanists, who reason from a much larger area of observation. Such authorities as Nägeli, Cienkowski, and de Bary are amongst those who maintain, in opposition to the pathological specialists, that wide range of form and wide range of physiological activity are possible in one species or "race" of Bacteria. To this subject I propose to revert in detail, on a subsequent occasion. As an example of the borrowing of methods by pathologists from botanists, I may quote the fact that it is customary in the writings of pathologists to attribute the gelatine method of cultivation to Dr. Koch, and to attach some additional weight to his conclusions on the ground that he has originated this and other ingenious methods of research. As a matter of fact, the gelatine method of cultivation, which is only a modification of the potato-slice method, is due to the botanist Brefeld (as acknowledged by Koch himself).

Whilst it appears that there has not been on the part of the pathologists engaged in the investigation of Bacteria such an acquaintance with, and appreciation of, the work of the botanists as would be conducive to sound conclusions, it is true that the chemists also have frequently failed in the same way. Much of the work of M. Pasteur on Bacteria is difficult, if not impossible, to verify or to use in any way, on account of the fact that he has not, in prosecuting his studies on these minute plants, made correct use of the conceptions and terminology of the botanists, and has on the other hand used that terminology erroneously and in a special sense.

Dr. Koch has given a very remarkable proof of the isolation of his knowledge and work from that of the botanists (among whom without question the most trustworthy conclusions in this department of knowledge are likely to be found) by his use of the term "spore" in his description of the tubercle-bacillus discovered by him. The "spore" of a bacillus, as shown more especially by the minute studies of the botanist Oscar Brefeld, is a very special structure formed within the filament of the bacillus by a modification of a *part* of its protoplasm, and provided with its own special capsule. Koch actually describes the whole of the constituent protoplasm of a tubercle-bacillus which has a moniliform arrangement as a series of "spores," although it is quite clear that there is nothing in common between the arrangement of the entire protoplasm of a bacillus in the form of a string of micrococci and the periodic and special elaboration of the spores of the hay and anthrax bacilli. The so-called "spores" of the tubercle-bacillus are spores only in the sense that all segments of bacteria which can be detached and multiply are spores (Arthrosporeæ of de Bary) and do not justify the distinction which Koch makes when he states that the tubercle-bacillus is characterised by producing spores, whilst stating that spirilla, such as the spirillum of relapsing fever (which breaks up into segments capable of growth), do not produce spores.

Bearing in mind these facts as to the attitude of different schools of bacteriologists, let us examine the claim put forward by Dr. Koch, and on behalf of Dr. Koch, by the writer in NATURE of December 4, p. 97, to have discovered that a certain comma-shaped bacterium is the cause of cholera. The writer in NATURE gives a summary of the various peculiarities of growth, form, and properties which Dr. Koch states he observed to be characteristic of a micro-organism occurring in the intestine of persons dead of cholera. He then observes: "Micro-organisms presenting all these characteristics are the bacilli described by Koch; organisms presenting only some of the characteristics, such as microscopical appearance, but differing in other points, are not Koch's comma-bacilli." To this conclusion, it is quite impossible in our present state of knowledge to assent. Its acceptance by the writer of December 4 renders it improbable that he will ever be convinced that Dr. Koch has formed an erroneous conclusion. The pretension put forward on behalf of Dr. Koch amounts to this, viz., that he has ascertained all the properties of this organism, that he cannot possibly have made any mistake, and that it is more probable that this organism has, since Dr. Koch left India, disappeared from existence, and been replaced by another very much like it, but not quite the same, than that any subsequent observer should be able to correct the hurried observations made by Dr. Koch when he was there. Such a pretension, were it advanced in regard to an animal or plant belonging to a group of exceedingly well-defined and highly-organised species would be unreasonable, but when put forward in relation to a representative of a group consisting of such minute, unstable, protean, and ill-understood species as are the Bacteria, must lead us to question altogether the impartiality and critical faculty of those who make it.



A, Outline of the bacillus of glanders (which Koch says resembles the comma F). B, Diagram of *Bacillus subtilis* of hay infusion during sporulation; *a*, sheath of the bacillus; *b*, transverse septum; *c*, coat of a spore; *d*, content of a spore; *e*, protoplasm surrounding the spore, which disappears entirely when the latter is fully formed; *f*, empty or sterile segment. C, Tubercle bacillus; the protoplasm is arranged in moniliform masses (*e*), which are erroneously called "spores" by Koch. D, Diagram of hay bacillus in vegetative state; the protoplasm is arranged in block-like masses (*e*), comparable to the moniliform masses of C. E, Spirillum dividing into commas. F, Commas (stated by Koch to be identical in form with the glanders bacillus, fig. A).

Admitting, however, for a moment that Dr. Koch's comma-bacillus is as peculiar as he supposes, admitting, as Dr. Koch originally implied by his silence as to the existence of other comma-shaped bacteria, that it is utterly unlike anything at present known in shape as well as in its action on gelatine, Dr. Koch has not proved or even rendered it greatly probable that this comma-bacillus is the cause of cholera, even when we accept his statement that "he has always found the comma-bacilli constantly accompanying cholera, and that he has never found them elsewhere." In the first place, it is quite certain both from Dr. Koch's reports and from the observations of others, that cases of cholera occur in which these commas are not abundant, in fact are insignificant in quantity; and in the second place, great as has been Dr. Koch's activity in the study of Bacteria, the fact that he and others with whom he is in relation have not found the "comma-bacilli" elsewhere does not render it at all improbable that other observers might find them elsewhere. This fallacy, viz., as to the perfection of Dr. Koch's knowledge of all possible forms and modes of occurrence of Bacteria, I have already pointed out above.

On the supposition that these comma-bacilli *never* occur except in the choleraic process it is of course impossible to maintain (see the article in NATURE of December 4, section (7), p. 98) that the choleraic process merely favours the growth of the commas. But Dr. Koch admits that they occur and flourish outside the human body, in immediate connection with cholera dejecta; also that, when artificially cultivated, they flourish on substances not derived from the human intestine. What proof is there that they do not naturally continue so to flourish? Dr. Koch offers none—he merely tells us that he has failed to show that they do. It is not at all impossible, on Dr. Koch's own showing, that they do—and if they do, what becomes of the argument as to the impossibility of their introduction from external non-choleraic conditions into the human body?

The suggestion is also considered by Koch (and is cited in the section of the article already mentioned) that, "as a result of the disease (cholera), conditions arise which cause the transformation of some ordinary bacterium into comma-bacilli." But, say Koch and his English disciple, there is no evidence of such rapid transformation of one form of bacterium into another. Here we meet with the special axiom of the pathologists to which I have already referred. The opinion of those who are entitled to the very greatest consideration, namely, the botanists Nägeli, Cienkowski, and de Bary, is that there IS evidence of such rapid transformation of one form of bacterium into another. Without going further than the case cited by the writer in NATURE as "merely" an alteration in pathogenic action, we have the instance of the attenuation of the virulence of anthrax bacilli, and we have also the case of the complete change of form of that same bacillus into nostocoid chains of spherical elements when cultivated on pork broth as shown by Klein. These two cases are by no means isolated ones (see my own researches on *Bacterium rubescens*, and also those of Zopf), but were they so they would be sufficient to establish the possibility of such changes in other Bacteria and to destroy the argument based on the assumption that such change is impossible.

The "only conclusion which remains" (see paragraph (*c*), section (7), in the article referred to) is therefore NOT that these bacilli and the cholera processes stand in the relation to each other that the commas are cause and the cholera effect. On the contrary, the only conclusion which remains is that WE DO NOT KNOW whether the commas although not detected by Koch may not be present in some parts of the healthy body, or flourishing outside it on organic matter, or may be the result of the transformation of some other bacterium, or may be the cause of cholera.

And the only way in which that ignorance can be removed has been very clearly recognised by Dr. Koch and all other recent writers, previous to the attempt made by Koch in 1884 to persuade the medical and scientific world that he had discovered the cause of cholera. The obscurity and uncertainty surrounding the Bacteria is such that no value can be attached to any asserted connection of a micro-organism with a disease as the cause of that disease, which is not based upon the experimental production of the disease by the inoculation into healthy animals of "pure cultures" of the suspected micro-organism. Dr. Koch's earlier statements on this subject are so precise and apt that I cannot do better than quote them here. He says in a pamphlet published in 1882, entitled "Ueber die Milzbrandimpfung":—

"The position which I take up is briefly as follows:—It is not yet proved that all infectious diseases are caused by parasitic micro-organisms, and consequently in each particular disease the proof of the parasitic character of the disease must be furnished. The first step towards this proof consists in the careful investigation of all those parts of the body affected by the disease, in order to establish the presence of the parasites, their distribution in the diseased organs, and their relation to the tissues of the body. . . . It is not until a thorough knowledge has been obtained in this way as to whether micro-organisms are present in the diseased parts, at what points they are present in perfect purity—whether, for instance, in the lungs, spleen, heart's blood, or elsewhere—that the attempt can be made to obtain the proof that these micro-organisms are of a pathogenic nature, and that they are more especially the cause of the disease in question. With this object in view, they must be isolated by means of 'pure cultivation,' and when they have been freed in this manner from all particles of the diseased body originally adhering to them they must be introduced by inoculation into the same species of animal in which the disease was observed, or, if that should not be possible, into animals in which the disease in question is known to occur with unmistakable symptoms. . . . An example is afforded by the disease known in man as erysipelas. It has been known for a long time that in this disease micrococci constantly are found in the lymph-vessels of the skin. *But by this knowledge it certainly was not proved that the micrococci are the cause of erysipelas.* Now, however, that Fehleisen has recently succeeded by excision of portions of skin from erysipelas patients (with every precaution against contamination by other bacteria which might be accidentally present on the skin) in rearing these micrococci in 'pure cultivations,' and in producing typical erysipelas by inoculating the human subject with these isolated micrococci, there can no longer be any doubt that the micrococci are, in fact, the cause of erysipelas, and that the latter is to be regarded as a parasitic disease."

This is the kind of proof which we require in the case of the comma-bacillus, and its supposed causal relationship to cholera. Dr. Koch has not succeeded in obtaining that proof. He has tried, and has failed, to produce cholera by inoculation of "pure cultivations" of his "comma." Cholera, at present, is not known as a disease in animals. Nevertheless, Dr. Koch has urgently and persistently declared that he considers it to be proved that the comma-bacillus is the cause of cholera! After repeated and public declarations of this conclusion, he is now making experiments by introducing his "comma-bacillus," not through the mouth, but by fistulæ into the intestine of rodents. Those who know the history of experiments on the production of cholera in mice and other rodents will not be convinced, even should Dr. Koch succeed in producing choleraic symptoms in this manner, since the readiness with which cholera-like processes are induced in these animals by abnormal conditions is such as to render them unfit subjects for these researches.

II. We may now revert to some of the statements made by Dr. Koch, which in the preceding remarks we have accepted without criticism. Even when this method is pursued, we find Dr. Koch's conclusions unwarrantable; they will appear still more so when we examine his position in detail. The writer of the article in NATURE of December 4 has omitted to notice a very important charge brought by Dr. Lewis against Dr. Koch, after the publication of Dr. Koch's address to the Medical Conference at Berlin in last August. Dr. Koch, also, has remained entirely silent in regard to this matter. It would be a very important thing if he would even now frankly reply to it. Dr. Koch and his defender assert that the "comma-bacilli" were found by Dr. Koch in cholera cases in Egypt, and also in specimens of intestine sent to him from India previous to his going there. Dr. Timothy Lewis, on the other hand, asserts that Dr. Koch had *not* recognised the "comma-bacillus" previously to his visit to India, and that in Egypt Dr. Koch attributed the causing of cholera to a totally different organism from that which he put forward after his arrival in India, and that, although he had thus shifted his ground, Dr. Koch did not admit at the time, and has not since admitted, that he was at one time convinced that cholera was caused by one organism, and a few months after was convinced that it was caused by another.

This charge is of importance for two reasons. If true, it must tend to lessen the confidence reposed by some in Dr. Koch's conclusions; and, secondly, it must also lessen our belief in the candour with which he states all the circumstances attending his observations and inferences.

The following quotations from the official reports sent home at intervals by Dr. Koch, coupled with the fact that he has not replied to Dr. Lewis on this point, though he has replied to him on other points, seems to leave little room for doubt that Dr. Lewis is perfectly correct in the very grave charge which he has brought against Dr. Koch.

In his report from Alexandria, September 17, 1883, Dr. Koch says:—"These bacteria are rod-shaped, and belong accordingly to the genus bacillus; they resemble most nearly in size and form the bacilli found in glanders" (which are straight: see woodcut, fig. A). In his report from Calcutta, dated January 9, 1884, he says:—"The microscopic examination demonstrated the presence of the same bacilli in the cholera intestine as had been found in Egypt." In a further report, dated February 2, 1884, we at last get the following remarkable statement:—"The bacilli are not quite rectilinear, like other bacilli, but slightly curved, like a comma. The curvature is sometimes sufficient to give the bacillus a semicircular form" (see woodcut, fig. F).

I think that it is abundantly clear that the organism selected by Dr. Koch in Egypt as the cause of cholera is not the same organism as that which he selected when in India, and that, although he is aware of that fact, he has not explicitly stated it, but has on the contrary (as does the writer in NATURE) endeavoured to give the impression that they are the same organism.

A further point of great importance as affecting the validity of Dr. Koch's theories, with regard to the connection of what he calls the comma-bacillus with cholera, is the statement of Dr. Lewis which is abundantly confirmed, and is not disputed by Koch, viz. that comma-bacilli, indistinguishable in appearance from those occurring in cholera cases, are quite common in the mouths of healthy persons. There is no doubt whatever that this is the case, although no record of the fact is to be found in any published treatise or paper on Bacteria, and that it was not commonly known to bacteriologists previously to Dr. Lewis's announcement of it in last September. The writer of the article in NATURE of December 4 hardly gives full effect to the importance of this point, since he cites Dr. Koch's reply to Dr. Lewis at the same time that he records Dr. Koch's earlier statements. Setting aside for the moment Dr. Koch's reply to Dr. Lewis, let us examine Dr. Koch's statements bearing on this subject, at the time when he announced his supposed discovery of the cause of cholera. He wrote from India that the organisms which he identified as the cause of cholera were of *peculiar* form, and "on account of its peculiar form, I have given to it the name of comma-bacillus." Throughout his subsequent writings, previous to the publication of Dr. Lewis's report by the Army Medical Department, Dr. Koch speaks of his cholera-organism as *the* comma-bacillus. He does not mention that any micro-organism similar to it in form is known to him. Had he been acquainted with one commonly occurring in the mouth, he would certainly have said, "The cholera comma is very like one occurring in the mouth, but differs in such and such ways." So far from this, he expressly says that no similar organism occurs in the human body, and states that he has failed to find an organism like the comma-bacillus in (amongst other places) the human mouth. No subsequent statement (after Lewis's publication) can affect the evidence which we have here that Dr. Koch was not acquainted with the "comma" which occurs in the human mouth.

After Dr. Lewis had shown that a "comma-bacillus" indistinguishable from Koch's "comma-bacillus" occurs in the healthy human mouth, and that accordingly—if we may suppose, from their identity of form and close association, that the two organisms are identical in every respect—the fundamental proposition of Koch as to the exclusive association of his comma-bacillus with cholera utterly breaks down, Dr. Koch replied as follows—(1) that the occurrence of a comma-bacillus in the mouth had long been familiar to bacteriologists (he did not say, it is to be noted, that it had long been familiar to him); and (2) that this comma-bacillus of the mouth will not grow upon neutralised cultivating-gelatine, whereas that from the intestine will, and that accordingly there is no ground for regarding them as identical species.

It seems to me in the highest degree improbable that Dr. Koch was acquainted with the mouth-comma when he published his conclusions as to the cause of cholera. If he was acquainted with it, it is undeniable that he committed a very grave fault in not drawing attention to it, and pointing out then and there the differences presented by cultures of the two commas. I have fairly conclusive evidence before me of the fact that Dr. Koch was not acquainted with the comma-bacillus of the mouth two years ago, when he published his large report and coloured plates on the tubercle-bacillus. In one of the drawings in that work he gives a delineation of the chief forms of micro-organisms occurring in the mouth, in order as he says to enable other observers to guard themselves against any confusion of the tubercle-bacillus with the micro-organisms which are normally present in sputa. *No comma-like organism is figured in that drawing or mentioned by Dr. Koch.*

As to the cultures of the "comma" from cholera intestines on the one hand, and from the healthy mouth on the other, differing in respect of their properties or their sensitiveness to conditions of alkalinity and neutrality, I venture to say that, *taking into consideration the whole history of the case*, it is not sufficient for Dr. Koch to tell us in an abrupt way that such differences exist. There is no reason to accept as final and perfect Dr. Koch's account of the characters of the comma associated with cholera, and I should greatly prefer to have the comparison of the conditions of growth of the commas from these two sources made by some one who is not, as Dr. Koch must unfortunately be, so very seriously biased in one direction.

I think there is some reason to expect that we shall hear from Dr. Klein as to the result of his impartial experiments, now being carried on in Calcutta, that the comma which occurs in the healthy mouth behaves in precisely the same way under cultivation, and is in fact as in appearance the same organism as the comma which occurs in the intestines of cholera patients.

Lastly, I may record a protest against Dr. Koch's extraordinary term "comma-bacillus." I have already pointed out that Dr. Koch uses botanical terminology loosely. The word "bacillus" has been by common consent restricted to the description of such rod-like forms as Koch first associated with cholera as the result of his Egyptian work. To prefix the word "comma" to this, was perhaps a method of avoiding unpalatable explanations. At the same time it is utterly inconsistent with the sense of the words. What Koch calls "comma-bacilli" may for convenience be termed "commas." They are well known to botanists as the segments of a spirillum (see woodcut, fig. E), the result of the breaking up of a spirillum into little pieces, one corresponding to each turn of the spire. They have been clearly figured and their nature recognised by Zopf. The "commas" of the human mouth and intestine are undoubtedly related to a spirillum which is frequently found in association with them, and would not have caused any astonishment or been stigmatised as "peculiar" in form by an observer who had that adequate knowledge of the natural history of the Schizomycetes in general which Dr. Koch has in many ways shown that he does not possess.

E. RAY LANKESTER

[We desire merely to make one remark with regard to the foregoing letter. The article referred to was prepared at the request of the Editor with the view of putting before the scientific public a fair and complete statement of Dr. Koch's case. The writer of the article requests us to state that he did not, except in the last paragraph, give any views of his own, and holds himself perfectly neutral in the matter, his mind not being at all made up on the subject.—ED.]

On the Distribution of Honey-Glands in Pitchered Insectivorous Plants

THE four genera of pitchered insectivorous plants at present in general cultivation are Nepenthes, Sarracenia, Darlingtonia, and Cephalotus. Attention was drawn to the minute structure and physiological action of the first three of these by Sir J. Hooker in his celebrated presidential address to the British Association in 1874, while the structure and morphology of the last was treated of by my master, Prof. Dickson (*Journal of Botany*, 1878, 1881). Both observers pointed out an *attractive surface* studded with honey-glands, which constituted the lid part, a *conducting surface*, either of an exceedingly smooth nature (Nepenthes), or beset with small downward-directed hairs (Sarracenia, Darlingtonia, Cephalotus), and in most cases a *glandular surface* (Nepenthes, *S. purpurea*, and Cephalotus), the secretion from which directly or indirectly assisted in digestion of animal products. In Sarracenia and Darlingtonia there was found in addition a *detentive surface*, covered with long deflected hairs.

A year ago Prof. Dickson further drew attention to a set of magnificent attractive glands along the free edge of the corrugated rim in Nepenthes, which he named "marginal glands."

My attention has recently been directed to all the genera, and I propose stating here the main results. A detailed account of the comparative results obtained by examination of the different species in the young and adult condition will shortly be presented to the Royal Society of Edinburgh.

Nepenthes.—Examining a pitcher of Veitch's beautiful hybrid, *N. Mastersiana*, I observed on its outer surface what seemed to be the small openings of honey-glands. When microscopically examined, they were found exactly to resemble those on the inner lid surface, except that the gland fossa was deeply hollowed out, and opened externally by a small orifice, while its inner surface was clothed to within a short distance of the orifice